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Electoral Confidence and Political Budget Cycles in Non-OECD Countries

Max Ferdinand Eibl and Halfdan Lynge-Mangueira

Introduction

Common wisdom has it that governments boost public spending before elections to improve their re-election chances. Yet evidence of political budget cycles (PBCs) suggests that they do so only sometimes.¹ Most explanations as to why this is have focused on incumbents' abilities to manipulate expenditures. Fewer have focused on their incentives, and little research has tested the effect of their electoral prospects. This is certainly the case for studies of PBCs in non-OECD countries. While the effect of electoral prospects has been acknowledged in the literature on advanced industrialized democracies (Alt & Rose, 2009; Schultz, 1995), it has been almost completely absent from scholarship on developing countries.

In this article, we seek to fill this lacuna by exploring the relationship between electoral prospects and PBCs in non-OECD countries.² We argue that electoral confidence affects whether incumbents increase public spending before elections. Our argument highlights the importance of incentives and expectation formation on the occurrence and magnitude of PBCs. In short, we hold that if incumbents are confident of their re-election, they do not boost public spending; if they fear losing, they do.

Moreover, given the information-scarce environment prevalent in most non-OECD countries, we argue that incumbents rely primarily on past electoral results when gauging their re-election chances. Tested against alternative measures of electoral confidence, specifically favorable pre-election polls, incumbents seem to prefer past electoral results. While we acknowledge that incumbents in non-OECD countries use all information at their disposal, the political environment and the fact that decisions regarding election year spending are often taken many months before the elections, obliges them to focus primarily on previous electoral results. It is also possible that favorable surveys are the result of earlier spending decisions in response to previous election results, thus making opinion polls potentially endogenous to spending and electoral results. In any event, our findings suggest that incumbents prefer to rely on previous win-margins when deciding whether to boost spending or not.

We test our argument against a new dataset on public spending in non-OECD countries (Lucas & Richter, 2016). Covering 76 countries from 1960 to 2006, the dataset considerably improves on existing data, both in terms of country coverage and temporal scope. Existing studies on PBCs in developing countries have often been limited to specific geographic areas or time periods.³ This paper therefore represents, to the best of our knowledge, the most comprehensive test of political budget cycles in developing countries. Like Rogoff (1990) and others, we

focus on spending rather than revenues or debt.⁴ Our findings show that the occurrence and the strength of PBCs are conditional upon the incumbent's electoral confidence. More specifically, we find a negative relationship between past win-margins and pre-electoral fiscal manipulation. Incumbents who won the last elections by a comfortable margin and who are therefore confident of their re-election did not boost public spending before elections; incumbents who won only by a narrow margin and feared they could lose the elections manipulated the state budget and increased public expenditures. In other words, we provide evidence of a linear relationship between electoral confidence and spending cycles: the higher an incumbent's confidence, the smaller the spending increase before elections. Testing an alternative, survey-based measure does not yield the same results, suggesting that past win-margins represent a better indicator of incumbents' electoral confidence in the context of non-OECD countries.

The article proceeds in four sections. The first section reviews the existing PBC literature and highlights the neglect of incentives and the effect of electoral prospects outside the context of OECD countries. The second section discusses the formation of electoral prospects in political contexts outside the OECD. The third section describes the dataset and uses quantitative methods to examine the effect of electoral confidence on state spending. The fourth section concludes by highlighting the added value of this study.

Neglected Incentives for PBCs

PBCs in developing countries have often been approached as a moral hazard problem (Shi & Svensson, 2006). The model assumes that incumbents control public spending and value re-election, and that voters value the goods and services that public spending buys.⁵ Voters are impaired by asymmetric information, meaning that they cannot discern whether more public goods stem from incumbents' competence or from fiscal manipulation. The result is a cycle where incumbents increase public spending before elections to improve their re-election chances, and reduce spending thereafter to prevent deficits and inflation.⁶

The argument behind the model is compelling. Yet evidence of PBCs is mixed. Alesina, Cohen, and Roubini (1992; 1997), and Alt and Lassen (2006) have found PBCs in OECD countries, while Schuknecht (1996, 1999) has found PBCs in developing countries. Vergne (2009) has also found PBCs in developing countries but only in certain types of spending. In datasets containing both OECD and non-OECD countries, Brender and Drazen (2007) have found some evidence of PBCs, while Klomp and de Haan (2012) have found no evidence at all.

In view of these mixed findings, PBC research has increasingly turned to exploring the conditions under which incumbents engage in fiscal manipulation (Alt & Rose, 2009; Franzese, 2002; Hibbs, 2006). Most of this research has focused on incumbents' *abilities* to manipulate public spending. Persson and Tabellini (2003) and Franzese (2002) have highlighted institutional features of political systems that facilitate or hamper fiscal manipulation.⁷ Others have suggested that democratization reduces this ability, pointing to democratic stability (Adi Brender & Drazen, 2007; A. Brender & Drazen, 2005), voter experience (Akhmedov & Zhuravskaya, 2004; Shi & Svensson, 2006), party institutionalization (Shelton, n.d.), and fiscal transparency (Alt & Lassen, 2006). Introducing an international dimension to PBC research, Hyde and O'Mahoney (2010) and Schuknecht (1996) have shown that IMF agreements reduce incumbents' ability to manipulate public spending.

Far fewer studies have focused on *incentives*. Ito (1990) and Chowdhury (1993) have argued that the ability to call early elections (endogenous timing) lowers incumbents' incentive to create PBCs, as they can call elections when natural spending cycles are favorable. Alesina, Cohen, and Roubini (1993) have tested this argument cross-nationally but found no evidence to support it. Kayser (2005) has suggested that term limits reduce the incentive to manipulate, although he also finds little evidence to corroborate his claim. Finally, Shi and Svensson

(2006) have argued that budget transparency reduces incumbents' ability to appropriate rents while in office and therefore their incentive to create PBCs. An alternative interpretation of their findings is that transparency requirements simply reduce incumbents' ability to manipulate budgets and thus has nothing to do with incentives.

The effect of electoral prospects and incumbents' electoral confidence has rarely been tested. This is surprising as the argument dates back to the early days of PBC research. Already Rogoff (1990) notes that political cycles should not be observed when incumbents are safe. Similarly, Alesina, Cohen, and Roubini (1993, p. 21), in one of the first tests of the Rogoff model, speculate that PBCs "occur when incumbents are unsure of reappointment and need an extra electoral boost." The argument has been tested in the context of OECD countries but never cross-nationally. Schultz (1995) and Price (1998), for example, use public opinion data to test the effect of government popularity on election year spending in the UK. They both find that government popularity affects spending but disagree on how.⁸ Similarly, Alt and Rose (2009) find that approval rates influence US governors' decision to spend before elections. Aidt, Veiga, and Veiga (2011) and Pettersson-Lidbom (2001) use past win-margins to measure the effect of competitiveness on election year spending in Portuguese and Swedish municipalities, finding that local governments hike up spending when previous

elections were close. Schneider (2010) tests the same effect in German federal states but finds no statistically significant effect. The argument has never been tested outside the context of OECD countries but its logic should be the same. All else being equal, incumbents should have a stronger incentive to manipulate public spending when their electoral prospects are poor, and a weaker incentive when they are confident of their re-election.

Formation of Electoral Prospects in Different Contexts

The argument raises an important question: how do incumbents know about their electoral prospects? In OECD countries, they have a fairly good idea. Since the late 1930s and early 1940s, companies such as Gallup have conducted pre-election polls across Western Europe and North America,⁹ and by the early 1980s, commercial polls were conducted in nearly all Western democracies (Crespi, 1989). Today, polls are copious and easily available. In the run-up to the 2012 US presidential elections, for instance, more than 300 polls were conducted (RCP, 2013). In addition, electoral sociology tends to be more advanced in OECD countries. Candidates have a fairly good understanding of voter preferences and electoral behavior, and use focus group interviews to test the effect of different messages before conveying them to the public. Finally, they draw on statistical models to predict electoral results on the basis of demographic and economic data.

This is not the case in most non-OECD countries. As Heath, Fisher, and Smith (2005, p. 311) note, survey research “is not yet completely global” and certain areas, notably the Middle East and sub-Saharan Africa, have until very recently remained underrepresented in international public opinion research. Even in long-standing developing democracies, such as India, polls were often non-existent before the 1990s. The polls that are conducted in non-OECD countries are often fraught with problems regarding coverage, nonresponse rates, sampling, and measurement. Illiterate and rural populations, in particular, tend to be underrepresented in most polls (Heath et al., 2005). For example, in Egypt in 2012, companies such as the Pew Global Attitudes Survey sampled respondents only from the greater Cairo area, which may have been one of the reasons why virtually all polls predicted a run-off between two candidates who both did not make it to the final round.¹⁰ Similarly, in Zambia in 2006, polls conducted mainly in urban areas predicted a landslide victory for the opposition challenger, prompting incredulity among his supporters when the incumbent president was declared the winner (Larmer & Fraser, 2007). Representative sampling may also be complicated by armed conflict, political unrest, or poor infrastructure, which makes regions inaccessible for pollsters. Finally, laws regulating public opinion polls are often highly restrictive.¹¹

The lack of reliable information is particularly heightened in the case of founding elections. Returning from periods of non-democratic rule, incumbents are highly uncertain about the ability of their party machines to deliver the votes. Voters may use the elections to sanction old regimes and members of the political elite may seize the opportunity to defect from the hitherto ruling party. Unprecedented levels of voter turnout following democratic openings exacerbate this uncertainty. In such insecure environments, incumbents desperately hunt for reliable information. In Ghana, for instance, the ruling National Democratic Congress solicited the Ghana Private Road Transporters' Union prior to the 1992 elections, which marked Ghana's return to multiparty democracy, and had taxi drivers all over the country listen in on their passengers' conversations (Jeffries & Thomas, 1993).

Overall, incumbents in non-OECD countries face a dearth of information when they try to assess their re-election prospects. In this information-scarce environment, we hypothesize that incumbents rely on past electoral results to gauge their re-election prospects. If they won the last elections by a comfortable margin, they are unlikely to spend much; if they won by a narrow margin, they are likely to manipulate budgets to improve their re-election chances. By comparison, opinion polls do not provide the same quality of information. First, they are often not available when decisions regarding election year spending are made, usually

many months before the elections. Second, even if polls were available, they may be inaccurate and fraught with problems, as described above.

This is not to say that incumbents *only* rely on past win-margins when they gauge their electoral prospects. They will most certainly use all information available to them and consider the shape of the economy, assess the impact of possible economic shocks, listen to party members, analyze the press, and assess the implications of possible corruption or mismanagement scandals. In the end, the whole process is about evaluating and weighing information according to its reliability. We simply hypothesize that the specific context of non-OECD countries entails that the informational weight of past electoral results is higher than the insights gained from pre-election polls, if available. Incumbents thus follow a pattern of Bayesian learning under conditions of poor information.¹²

Anecdotal evidence supports this argument. In Ghana in 2004, GDP growth rates were high, above 4 percent in the years before elections, and polls consistently predicted a victory for the incumbent, the National Patriotic Party (NPP)'s John Kufuor. The government, nonetheless, increased the budget deficit from 8 percent of GDP in the year before the elections to 9.2 percent in the year of the elections. This increase should be seen in the light of Kufuor's 13.8 percent win-margin in 2000, which may seem relatively comfortable but, in fact, was the

smallest in Ghana's history and the first time a government was defeated at the polls.

Conversely, in Trinidad and Tobago in 1992, polls suggested that the ruling National Alliance for Reconstruction (NAR) would lose its 91.7 percent majority in parliament (33 of 36 seats). The year before the elections, the economy went into recession, which made it more difficult for the government to improve the budget balance. This notwithstanding, the government slashed spending by nearly 1.6 percent in the year of the election.

Even in the more advanced economies, where polls are more frequent and reliable, there are indications that incumbents weigh past results as much as they weigh pre-election polls. In Cyprus in 1998 and 2003, the polls predicted a victory for the incumbent president, the Democratic Rally's Glafcos Clerides. Yet, spending hikes of 1.2 percent of GDP in 1998 and 1.8 percent in 2003 suggest the government looked more at its 0.6 percent win-margin in 1993 and 1.6 percent win-margin in 1998 when they set the spending level before the 1998 and 2003 elections.

Analysis

This section explores the relationship between incumbents' electoral confidence and pre-electoral fiscal manipulation, using time-series cross-sectional analysis of spending data in 76 non-OECD countries, 1960-2006. The findings support our argument that electoral confidence has a major impact on both the occurrence and size of PBCs in non-OECD countries. As highlighted above, we argue that poor and insufficient information and, by consequence, the inability of incumbents to estimate their re-election chances characterize the strategic environment in most of the countries in our sample. We therefore expect incumbents to use past electoral results rather than pre-election polls to gauge their re-election prospects and, by consequence, to decide whether to manipulate budgets or not.

Data

To operationalize our dependent variable, we use a new dataset on Global State Revenues and Expenditures (GSRE) (Lucas & Richter, 2016) in developing countries. This dataset draws on the country reports that are prepared annually by the regional departments of the International Monetary Fund (IMF), stored in the IMF's archives. The documents were made available to researchers in the early 2000s. The reports are generally available from the year in which a country joined

the IMF, while the most recent documents are declassified after a period of five years.

The GSRE is thus an alternative data source to the widely used IMF Government Finance Statistics (GFS), published by the IMF's statistical department. Compared to the GFS, the GSRE substantially increases the number of available observations by extending the time series back in time, for some countries until the end of World War II, while the GFS only dates back to 1972. Moreover, the GSRE provides one continuous time series of comparable data, while the GFS, due to changes in the IMF's classification scheme, has one time series running from 1972 to 2001 and another from 1990 until present. Another advantage of the GSRE is the arguably higher validity, as the data are collected by country teams during field missions as opposed to surveys sent out for the GFS. Admittedly, a disadvantage of the GSRE is that different regional departments might apply different accounting standards that could lead to distortions of the measures across countries. This is certainly the case for specific taxes or expenditures. However, for aggregate variables, such as budget deficit, the distortions are minimal as different spending classifications matter much less. In all, the GSRE represents a valuable alternative to the widely used GFS.¹³

Variables

Our main dependent variable, *Budget Balance*, represents the difference between total government revenues minus total expenditures, measured as a share of the country's GDP. Negative values of the budget balance thus designate fiscal deficits; positive values surpluses. Data are all taken from the GSRE with two minor exceptions. For periods in the 2000s where government data from the archival material are not available yet, we supplement with data from the GFS. We also use GFS data for India which is not included in the GSRE.

The variable *Election* is based on the National Elections Across Democracy and Autocracy (NELDA) dataset (Hyde & Marinov, 2011) and indicates whether a competitive, politically important election occurred in a given country-year. We restrict our analysis to “competitive” elections as our argument assumes some risk of being voted out of office. When electoral results are rigged or opposition candidates and parties outlawed, elections do not represent a test of an incumbent's popularity and our argument does not apply. We therefore exclude elections when no opposition was allowed, only one party was legal, or there was no choice of candidates on the ballot.¹⁴ Countries that have never had a competitive election are also not part of the sample. In addition, we exclude all countries from the estimation with a Polity score (Marshall & Jaggers, 2010) of -6 and below.¹⁵ In these clearly autocratic cases, meaningful political competition is

very unlikely to be guaranteed, even when elections are formally competitive, as hegemonic autocrats have many ways to vitiate competition before elections occur (Schedler, 2002). We also restrict our analysis to “politically important” elections, meaning elections where the chief executive’s office is at stake.¹⁶ Given limited resources, we expect PBCs to be particularly pronounced when the leadership of a country is at stake.

Our main conditional variable, *Confidence*, measures the incumbent’s win-margin in percent in the last elections. It ranges from 0 to 100, with higher values indicating higher levels of confidence. In the case of presidential elections, the variable measures the difference in vote share between the winner and the runner-up in the last elections. If elections are held over two rounds, we take the difference in the second round. In the case of parliamentary elections, we take the difference between the government and the opposition’s seat share in parliament. We use data on seat shares instead of vote shares as the latter are often unavailable. We acknowledge that, depending on the electoral system, there may be considerable distortions between the two. However, as seat shares, not vote shares, ultimately determine who holds power, we believe the use of seat shares is justifiable. This being said, we also run the regressions without parliamentary elections as a robustness test (Online Appendix, Table A4).

Data on electoral results are taken from the Election Results Archive (Center of Democratic Performance, 2012), Psephos (Carr, 2013), and the World Bank (2013). As the variable seeks to capture incumbents' confidence of being re-elected, it takes the value of zero whenever the elections are the first multiparty elections in a country or whenever they are the first competitive elections after at least one round of non-competitive elections. As we have argued above, in both cases incumbents are believed to have very little or no reliable information and, as a result, are expected to be very insecure about their electoral prospects. The variable is also coded zero in non-election years, as it is meaningless to measure the incumbent's fear of losing an election in a non-election year. This coding follows the standard practice in the field (see, for instance, Efthymoulou, 2011). That being said, we run the regressions also with a continuous measure of confidence as a robustness test (Online Appendix, Table A8).

We also test an alternative measure of electoral prospects, *Favorable survey*. To construct this measure, we first collected data on 147 pre-election polls, covering 50 countries over the period from 1961 to 2009. The data were post-weighted, according to geographical region and time period (pre- and post-1991) to reduce biases stemming from non-random sampling. Taking the difference between the predicted win-margin in the poll and the actual win-margin as a dependent variable, we then estimated a model of poll accuracy using

country-level characteristics, election characteristics, and decade dummies as predictors.¹⁷ From this model, we extracted an error term, which we normalized and interacted with a binary variable from the NELDA dataset (Hyde & Marinov, 2011) indicating whether there were reliable polls favorable to the incumbent.¹⁸ By interacting the NELDA dummy variable with our error prediction, we get a more fine-grained, survey-based measure of electoral confidence that – importantly given the context – takes into account the expected quality of polls in a given election year. This procedure is admittedly not perfect. Ideally, we would have fine-grained survey data for all elections in our sample that we could average over a defined period prior to the election. Unfortunately, exact poll data are hard to come by: even the 147 pre-election polls that we identified only represent about 60 percent of the cases in which according to NELDA reliable surveys existed, and NELDA itself only provides the survey variables in binary form – favorable or not. Failing better alternatives, we thus follow a strategy of weighting the NELDA survey dummy by the expected error margin of the survey.

The analysis also includes a number of standard socio-economic and political control variables. First, we include a variable measuring *Executive oversight*, taken from the Varieties of Democracy (VDem) dataset (Coppedge et al., 2015), which controls for an incumbent’s ability to manipulate public spending. More specifically, the variable measures the likelihood that illegal or

unconstitutional activity by the executive would be investigated by a body other than the legislature. Second, we control for *GDP per capita (logged)*, taken from the Penn World Tables (Heston, Summers, & Aten, 2006), as governments in wealthier countries might find it easier to finance higher deficits in election years by exploiting their larger tax base or turning to international markets. Third, we control for *Growth*, also taken from the Penn World Tables (Heston et al., 2006). The argument here is two-fold: on the one hand, economic growth is likely to affect the budget balance through higher revenues from taxes, again making it easier for governments to boost spending before elections; on the other, growth is likely to affect the incumbent's popularity and therefore electoral confidence as voters might reward the government for its economic performance. In other words, poor growth rates or recession could increase the incentive to create PBCs.¹⁹

Fourth, as varying levels of government revenues might affect the incumbent's ability to create PBCs, we include three variables that capture the government's distributive capacity: *Tax revenues/GDP*, taken from the GSRE (Lucas & Richter, 2016), which measures the sum of all direct and non-direct taxes weighted by a country's GDP; *Rents p.c. (logged)*, taken from Haber and Menaldo (2011), which measures government income from extractable resources

in 2007 \$US per capita; and *Aid p.c. (logged)*, taken from the World Bank (2010), which measures the level of foreign aid in constant \$US per capita.

Fifth, we include a dummy variable for *IMF* agreements, taken from an updated version of Dreher (2006), as the existence of such agreements might also reduce the incumbent's ability to manipulate the budget. Sixth, the incumbent's ability to create PBCs might be affected by a country's debt ratio, which is why we control for *Debt service*, taken from the World Bank (2010) and measured as a share of the gross national income. Seventh, as already explained, we exclude uncompetitive elections but in addition include a *Polity* indicator (Marshall, Gurr, & Jaggers, 2010), to make sure the results are not driven by regime variations. Eighth, with the variable *Tenure*, taken from Archigos dataset (Goemans, Gleditsch, & Chiozza, 2009), we control for the number of years the incumbent has been in office, as it might be that more experienced incumbents are more skillful in manipulating public spending (Aidt et al., 2011). Finally, we control for post-election swing-back effects in spending by including a dummy indicator for *Post-election years*.²⁰

Model

We estimate our regression using the following standard model as suggested in the literature (Shi & Svensson, 2006):

$$Y_{it} = \beta_0 + \beta_1 Y_{it-1} + \beta_2 Y_{it-2} + \beta_3 Y_{it-3} + \beta_4 Election + \beta_5 Election$$

$$* Confidence + \beta_6 X_{it} + N_i + T_t + \varepsilon_{it},$$

where Y_{it} represents the budget balance, β_0 is a constant; Y_{it-1} , Y_{it-2} , Y_{it-3} are the one-, two-, and three-period lags of the dependent variable; X_{it} is a vector of control variables, N_i and T_t are country and period fixed effects, and ε_{it} is the error term. Three important points: first, as public spending is characterized by inertia, we include three lags of the dependent variable to purge the regression of serial correlation;²¹ second, we include country fixed effects to control for possible country-specific confounders,²² and time fixed effects to control for periodical shocks in the panel; third, we use robust standard errors to adjust for within-country correlation and deal with heteroskedasticity.²³

We estimate the regression using ordinary least squares (OLS). Autoregressive OLS models combined with country fixed effects make the parameter estimates potentially liable to bias (Nickell, 1981). With an average T of 14 years per country, the Nickel bias in our cases is rather small (7.1 percent),²⁴ and Beck and Katz (2011) have shown that alternative estimators can perform worse in the presence of long time series. When using a difference GMM model as a robustness test (Online Appendix, Table A10), the substantive findings

remain unchanged. Finally, please note that *Confidence* as a base term drops out of the regression as it is coded 0 in non-election years.²⁵

Findings

Table 1 presents our results. In Model 1, we estimate our base model without conditioning on the effect of electoral confidence. The coefficient of *Election* is negative and statistically significant. This suggests that, without factoring in incumbents' electoral confidence, competitive and politically important elections have a negative impact on the budget balance. On average, the government's budget balance is about 0.4 percent of GDP lower in election years. Given a mean budget balance of -5 percent in the entire sample, this means an 8 percent increase of deficit spending in election years. In other words, all else being equal, it seems that incumbents generally increase the deficit before elections. Compared to previous mixed findings, this clear evidence of PBCs certainly results from the much larger number of observations in our dataset and the inclusion of new countries. However, it is in line with previous research, such as Schuknecht (1996, 1999) and Vergne (2009), who have found evidence of PBCs in developing countries. Notably, the coefficient of *Post-election* is positive, statistically significant, and comparable in size to the *Election* coefficient. This suggest that governments tighten the belt in the year after the elections.

[TABLE 1 ABOUT HERE]

In Model 2, we estimate our model with the conditioning effect of electoral confidence. Again, the coefficient of *Election* is negative and statistically significant, while the interaction of *Election* and *Confidence* is positive and statistically significant. Supporting our argument, this suggests that higher levels of electoral confidence improve the budget balance in election years, whilst the effect of elections is still negatively significant. In other words, more confident incumbents create smaller deficits or larger surpluses in election years. At the lowest level of electoral confidence, the budget deficit grows by about 0.8 percent of GDP. This means a 16 percent increase of deficit spending, given a mean deficit of -5 percent in the sample. From there, the deficit shrinks by about 0.02 percent for every percentage increase of electoral confidence. So, by moving from a situation of very low confidence to a win-margin of 20 percent, the size of the PBC is reduced by about 50 percent on average.

[FIGURE 1 ABOUT HERE]

Electoral confidence does not only reduce the PBCs. At high levels of electoral confidence, the cycles disappear. This is best illustrated graphically. Figure 1 displays the budget balance as a function of varying levels of electoral confidence.²⁶ The solid line represents the average effect; the two dotted lines the

95 percent confidence bounds.²⁷ The graph also includes a histogram to illustrate the distribution of electoral confidence in our sample.²⁸ Clearly, most elections are won by a narrow margin, meaning that most incumbents are relatively insecure about their re-election, and it is indeed these incumbents who manipulate spending before elections.

For confidence levels up to a level of 22 percent, incumbents increase the budget deficit in election years. However, deficit increases shrink in size with higher levels of electoral confidence. Past 22 percent, the upper confidence bound crosses the zero line, suggesting that incumbents might actually reduce their budget deficits in election years. The solid line suggests that PBCs occur, on average, up to confidence levels of 48 percent. We draw two conclusions from this. First, in view of the lower confidence bound, growing levels of confidence reduce the *size* of the budget cycle. This effect should be observable nearly across the whole sample.²⁹ Second, for past win-margins above 22 percent, we cannot be certain that PBCs actually occur. In other words, higher confidence levels also affect the *occurrence* of PBCs, though such confidence levels are empirically relatively rare.

How, then, does our alternative measure of electoral confidence fare? To answer this question, we replicate our base model using *Favorable survey* instead of *Confidence*. The results are shown in Table 2. As expected, favorable surveys

do not have the same conditional effect on PBCs as past win-margins. The coefficient of the interaction term between *Election* and *Favorable surveys* is negative and not statistically significant, indicating that favorable surveys may, in fact, reinforce PBCs rather than weakening them. Conditioning on favorable surveys also pushes the *Election* coefficient below conventional levels of statistical significance, although it is likely this change results from the reduced sample size.³⁰ Substantively, these findings suggest that incumbents resort to past win-margins rather than surveys to gauge their electoral chances, and decide whether or not to boost spending before elections. As argued above, this seems realistic given that polls in developing countries are often fraught with problems and the fact that decisions regarding election year spending are usually taken months before the elections. Whilst we cannot fully ascertain this claim because we have no information on the date the survey was carried out, the usually lengthy budget making procedures make it more difficult to react to, at times, fast-changing survey popularity. We also cannot rule out the possibility of reverse causation, such that favorable pre-election polls are themselves the result of earlier spending decisions by the incumbent. That said, incumbents' little trust in surveys must definitely play a role as we have a number of low-constraints executives in our sample that certainly could, in theory, respond with discretionary spending outside the regular budget-making channels.

[TABLE 2 ABOUT HERE]

Robustness Tests

To ascertain the robustness of our findings, we conduct a number of additional robustness tests. First, following common practice, we include additional control variables: a dummy variable for minority governments, assuming that minority governments may have more difficulties passing an inflated election year budget; variables for the percentage of the population that is urban and dependent, assuming that large rural and dependent populations may force governments to spend more on infrastructure, education, and health, which could constrain their ability to create PBCs; and a variable for the percentage of imports and exports, weighted by the country's GDP, which might also affect the incumbent's fiscal leeway. All variables are taken from Hyde and Marinov (2011) and the World Bank (2010, 2013).³¹ The results are shown in Table 3.

[TABLE 3 ABOUT HERE]

Second, considering the emphasis on abilities in the literature on PBCs, we run a number of robustness checks on our ability control. Specifically, we replace *Executive oversight*, first, with *Legislative constraints*, which measures the extent to which the legislature is capable of exercising oversight; second, with *Constitutional respect*, which measures the extent to which the executive respects

the constitution; third, with *Judicial constraints*, which measures the extent to which the executive complies with court rulings and to which the judiciary is independent; fourth, with *Judiciary compliance*, which measures how often the executive complies with court decisions with which it disagrees; and, fifth, with *Judicial review*, which measures whether any court has the legal authority to invalidate executive decisions (see Table 4). All variables are from the VDem dataset (Coppedge et al., 2015).

[TABLE 4 ABOUT HERE]

Third, as some incumbents may choose the stick rather than the carrot, we rerun our base model with two additional control variables: *Repression*, which measures the level of political terror on a five-point scale; and *Party ban*, which measures whether any parties are banned, also on a five-point scale (see Table 5).³² *Repression* is taken from the Political Terror Scale dataset (Gibney, Cornett, Reed, Haschke, & Arnon, 2015); *Party ban* from the VDem dataset (Coppedge et al., 2015).

[TABLE 5 ABOUT HERE]

None of the robustness tests described above alters the substantive finding of this paper: if electoral confidence is low enough, incumbents create PBCs to

boost their chances of re-election. Additional robustness checks are detailed in the Online Appendix. These include a model without parliamentary elections (Table A4); a model with different model specifications using year fixed effects, country fixed effects, and random effects only (Table A5); a model using panel-corrected and Driscoll-Kraay standard errors instead of our baseline errors (Table A6); a model without outliers (Table A7); a model where we replace our measure of electoral confidence, which is coded zero in non-election years, with a continuous measure (Table A8); a model, where we exclude founding elections and elections after an autocratic interlude (Table A9); a difference GMM model to address potential Nickel bias (Table A10); and, finally, a model that includes an interaction term between our ability measure and elections to capture the restraining effect of *Executive oversight* in election years specifically. The results of these additional tests are all in line with the findings of our base model.

Conclusion

This paper has yielded three key findings. First, incumbents' electoral confidence conditions their decision to manipulate public spending before elections. The causal mechanism is straightforward. Motivation to engage in pre-electoral spending manipulation increases as elections become tighter and incumbents' electoral prospects become more uncertain. Conversely, if incumbents are confident that they will win, they are reluctant to waste relatively scarce public

resources to boost their popularity, which would make it difficult to govern after the elections. Electoral confidence therefore affects both the size and the occurrence of political budget cycles. Overall, our argument contributes to the growing literature on the context-conditionality of PBCs (Alt & Rose, 2009). Using a new, comprehensive dataset on public expenditures, this paper also represents the most comprehensive test of PBCs in non-OECD countries thus far.

The second finding concerns the way incumbents form expectations. While we have underlined that incumbents in non-OECD countries use all information available to them, we show that they give past win-margins a particular weight in their calculations. Favorable pre-election polls, on the other hand, do not seem to influence incumbents' decision-making when it comes to PBCs. As we have pointed out, this probably has to do with the problem of survey reliability in non-OECD countries, a possible reverse causation between earlier spending decision and polls, and the fact that budgeting, in general, precedes elections by considerable time span. Being a relatively "hard" and reliable measure in an otherwise information-scarce environment, past win-margins act as the most important indicator of incumbents' future electoral prospects.

Finally, this paper challenges some of the previous findings of PBC research. For instance, our research suggests that the pronounced spending cycles before founding elections are primarily a result of incumbents' weak electoral

confidence rather than a lack of voter awareness and experience (Block et al., 2003). More generally, we have tried to shift the debate from institutional abilities back to motivational incentives, which have been largely neglected in the literature on PBCs in developing countries. Future research should explore other incentives that condition PBCs, such as party types or voter preferences. Also, it would be worthwhile to unpack aggregate spending and explore what types of spending incumbents prefer to boost their popularity. Finally, still very little is known about the electoral impact of spending manipulations. The findings of this paper suggest that incumbents in non-OECD countries believe that spending influences the outcome of elections. However, this assumption remains to be tested.

¹ Following Alt and Rose (2009), we define PBCs as “regular, periodic fluctuation in a government’s fiscal policies induced by the cycle of elections.”

² We exclude all countries that are currently members of the OECD from the analysis. The group of countries we analyze is thus not entirely commensurate, yet certainly overlapping to a large extent, with economically less developed countries.

³ See, for instance, Block et al. (2003) on Africa and Akhmedov and Zhuravskaya (2004) on Russia. Hyde and O’Mahoney (2010) study only the period after 1990.

⁴ It is generally agreed that PBCs are stronger in spending than in revenues (Alt & Rose, 2009). Thus, if we do not find PBCs in spending it is unlikely that we will find PBCs in revenues.

⁵ We consider PBCs, patronage, and vote-buying distinct phenomena (with corresponding distinct bodies of literature) but recognize that they intersect when patronage and vote-buying is government-funded. Our analysis captures only this intersection. Whether budget increases are used to fund public goods (roads, schools, hospitals, etc.) or private goods (wages, pensions, jobs, etc.) is a question of budget allocation, which we do not analyze in this paper.

⁶ The alternative partisan model of PBCs (Alberto Alesina, 1987; Hibbs, 1977) assumes relatively cohesive political parties with clearly delineated ideologies and is therefore unsuited for studies of PBCs in non-OECD countries (Block, 2002; Vergne, 2009).

⁷ Persson and Tabellini (2003) argue that in parliamentary systems incumbents control both the executive and the legislature, making it easier to push through budgets and public accounts.

Franzese (2002) argues that the separation of powers that characterizes presidential systems prevent incumbents from manipulating public spending.

⁸ Schultz (1995) suggests that the effect is positive and linear (unpopular governments boost public spending, as they fear losing elections; popular governments do not), while Price (1998) argues that it is bell-shaped (very unpopular governments also do not boost public spending as the costs of hiking up spending to the required level outweigh the benefits of being re-elected).

⁹ First surveys occurred in the US (1936), the UK (1937), and France (1938), followed by Australia, Canada, Denmark, Switzerland, the Netherlands, West Germany, Finland, Norway, and Italy by 1946 (see Worcester, 1989).

¹⁰ In a survey conducted by the University of Maryland just before the elections, Amr Moussa was predicted to obtain 28 percent (obtained 11.13 percent), and Abdel Moneim Abdel Fotouh was given 32 percent (obtained 17.47 percent).

¹¹ Particularly problematic are restrictions that forbid the publication of polls prior to elections. These blackout periods vary from one day to up to four weeks in some countries. According to the World Association for Public Opinion Research (WAPOR), pre-election restrictions have increased, and in 2012 virtually every other country reported the existence of blackout periods before elections (Chung, 2012). This means that even if pre-election polls exist, incumbents might be unable to access the information that would allow them to gauge their re-election prospects.

¹² Note that we do not make any assumption about expectation formation of voters, which has been a matter of ongoing discussion in political science and economics (Franzese, 2002; Krause, 1997).

¹³ Correlation between both data sources is still high, generally in the area of 0.70 and above. This implies that both datasets can be used interchangeably for periods past 1972.

¹⁴ The three properties are coded in Hyde and Marinov (2011).

¹⁵ In the classification scheme proposed by Marshall and Jaggers (2010), the polity threshold of -6 separates autocracies from hybrid regimes.

¹⁶ In practice, this means that we do not consider parliamentary elections in presidential political systems. To select politically important elections, we combine data from the Arthur Banks Dataset (2011) on the type and the mode of selection of the chief executive.

¹⁷ Since there is no literature on cross-country predictors of survey accuracy, we selected our predictors inductively, bearing in mind theoretical plausibility and a high predictive power as measured by R-squared. The final R-squared was quite satisfactory with 49 percent of the error predicted by our model. Details on the model including a full list of variables are available in the Online Appendix (Table A3).

¹⁸ This binary indicator combines information from two variables in the NELDA dataset, namely Nelda 25 and Nelda 26.

¹⁹ However, a stumbling economy might considerably limit the incumbent's capacity to engage in discretionary fiscal policies, making the latter scenario somewhat unlikely

²⁰ A summary statistic of our variables, as well as a list of all included countries is available in the Online Appendix (Table A1 and A2).

²¹ An Arellano-Bond test suggests the inclusion of three lags to remove all serial correlation in the error term.

²² A Hausman test suggests the use of country fixed effects.

²³ Panel-specific heteroskedasticity was detected using a modified Wald test.

²⁴ The bias amounts to $1/T$.

²⁵ In the Online Appendix, we run an alternative model with a continuous measure of electoral confidence, carried forward from the last elections (Table A8). The substantive findings of this paper remain robust to this model adjustment.

²⁶ This graph follows the suggestions of Berry et al. (2012).

²⁷ The scale for the effect on the budget balance can be found on the left y-axis.

²⁸ The scale for the histogram can be found on the right y-axis.

²⁹ To be precise, we should see this effect in 95 percent of our cases.

³⁰ As the survey indicator is not available for all countries in the sample, we lose 225 observations and three countries when adding the variable.

³¹ These additional control variables are not included in our base model as they are considered less important in the literature and are therefore rarely included in standard models of PBCs.

³² Recall that we exclude elections where no opposition was allowed, where only one party was legal, or where there was no choice of candidates on the ballot. Effectively, *Party ban* therefore becomes a three-point scale, ranging from “yes, many parties are banned” to “no, no parties are officially banned”.

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Tables and Graphs

Table 1: Electoral Confidence and PBCs

	(1)	(2)
Budget balance t-1	0.542*** (0.049)	0.542*** (0.048)
Budget balance t-2	-0.054 (0.043)	-0.056 (0.042)
Budget balance t-3	0.117*** (0.036)	0.117*** (0.036)
Election	-0.004* (0.002)	-0.008*** (0.003)
Election*Confidence		0.0002** (0.0001)
Post-election	0.005* (0.002)	0.004* (0.002)
Executive oversight	-0.003 (0.003)	-0.003 (0.003)
GDP p.c. (logged)	-0.007 (0.008)	-0.008 (0.008)
Growth	0.079*** (0.020)	0.077*** (0.020)
Tax revenues/GDP	0.170** (0.071)	0.174** (0.070)
Rents p.c. (logged)	0.001 (0.001)	0.001 (0.001)
Aid p.c. (logged)	-0.003** (0.001)	-0.003** (0.001)
IMF	-0.0005 (0.003)	-0.0001 (0.003)
Debt service	0.0005** (0.0002)	0.0004** (0.0002)
Polity	-0.0004 (0.0004)	-0.0004 (0.0004)
Tenure	-0.001 (0.0004)	-0.001 (0.0004)
Observations	1,015	1,015

Note: Autoregressive OLS model with country and year fixed effects. Robust standard errors in parentheses. Constant and FE coefficients omitted from table.

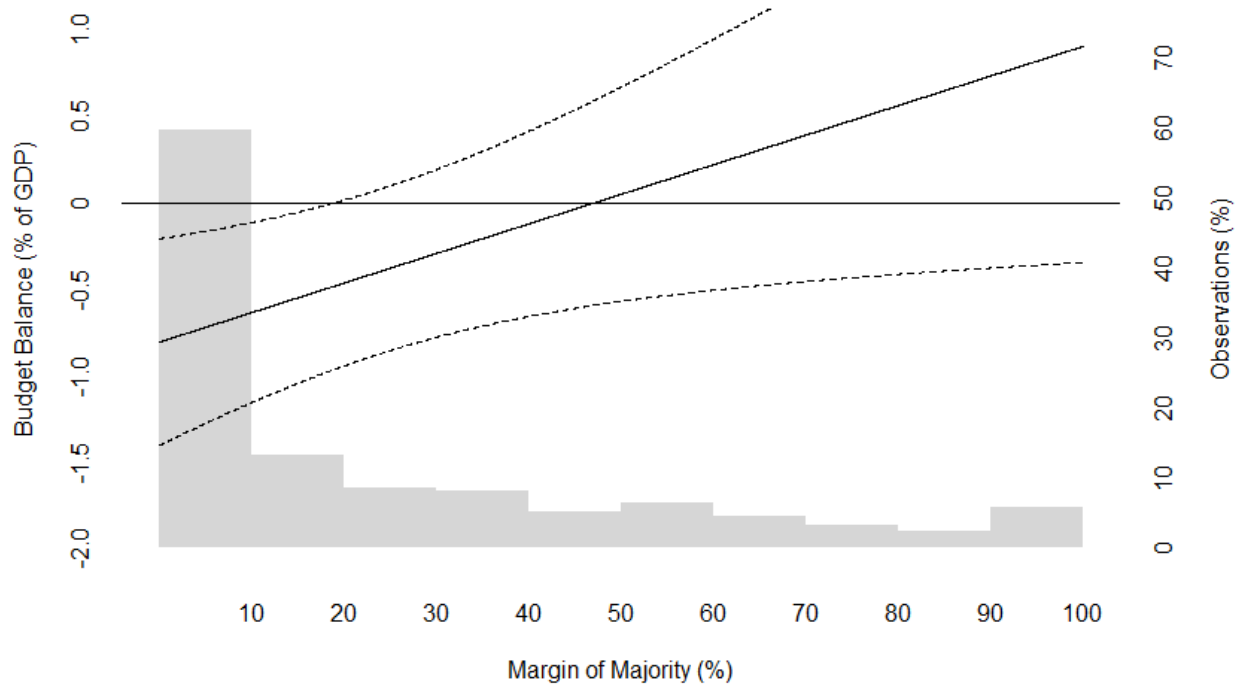


Figure 1: Marginal Effect of Electoral Confidence on PBCs

Table 2: Alternative Measure of Confidence

Budget balance t-1	0.510*** (0.047)
Budget balance t-2	-0.001 (0.042)
Budget balance t-3	0.106** (0.045)
Election	-0.004 (0.005)
Election*Favorable survey	-0.003 (0.005)
Post-election	0.007** (0.003)
Executive oversight	-0.013 (0.009)
GDP p.c. (logged)	0.098*** (0.019)
Growth	-0.001 (0.0004)
Tax revenues/GDP	0.0002 (0.0005)
Rents p.c. (logged)	-0.0001 (0.001)
Aid p.c. (logged)	-0.002** (0.001)
IMF	-0.0005 (0.003)
Debt service	0.169** (0.085)
Polity	-0.0005 (0.0004)
Tenure	-0.005 (0.012)
Observations	803

Note: Autoregressive OLS model with country and year fixed effects. Robust standard errors in parentheses. Constant and FE coefficients omitted from table. Surveys are weighted by expected measurement error.

Table 3: Additional Controls Variables

	(1)	(2)	(3)	(4)
Budget balance t-1	0.542*** (0.048)	0.538*** (0.048)	0.538*** (0.048)	0.538*** (0.048)
Budget balance t-2	-0.056 (0.042)	-0.054 (0.042)	-0.055 (0.042)	-0.055 (0.042)
Budget balance t-3	0.117*** (0.036)	0.124*** (0.039)	0.123*** (0.040)	0.122*** (0.039)
Election	-0.008*** (0.003)	-0.008*** (0.003)	-0.008*** (0.003)	-0.008*** (0.003)
Election*Confidence	0.0002** (0.0001)	0.0002** (0.0001)	0.0002** (0.0001)	0.0002** (0.0001)
Minority	0.002 (0.007)	0.002 (0.007)	0.002 (0.007)	0.002 (0.007)
Urbanization		-0.001*** (0.0002)	-0.001** (0.0003)	-0.001*** (0.0003)
Dependency			-0.001 (0.001)	-0.001 (0.001)
Trade				0.00003 (0.0001)
Observations	1,015	1,015	1,015	1,015

Note: Autoregressive OLS model with country and year fixed effects. Robust standard errors in parentheses. Constant, FE coefficients, and standard controls coefficients omitted from table.

Table 4: Alternative Indicators for Abilities

	(1)	(2)	(3)	(4)	(5)
Budget balance t-1	0.550*** (0.049)	0.552*** (0.048)	0.550*** (0.049)	0.548*** (0.049)	0.548*** (0.049)
Budget balance t-2	-0.055 (0.041)	-0.053 (0.041)	-0.055 (0.041)	-0.056 (0.041)	-0.054 (0.041)
Budget balance t-3	0.115*** (0.035)	0.114*** (0.035)	0.115*** (0.035)	0.114*** (0.035)	0.112*** (0.036)
Election	-0.008*** (0.003)	-0.007*** (0.003)	-0.008*** (0.003)	-0.008*** (0.003)	-0.008*** (0.003)
Election*Confidence	0.0002** (0.0001)	0.0002** (0.0001)	0.0002** (0.0001)	0.0002** (0.0001)	0.0002** (0.0001)
Legislative constraints	-0.003 (0.018)				
Constitutional respect		0.003 (0.003)			
Judicial constraints			-0.004 (0.014)		
Judiciary compliance				-0.005* (0.003)	
Judicial review					-0.008 (0.006)
Observations	1,050	1,050	1,050	1,050	1,050

Note: Autoregressive OLS model with country and year fixed effects. Robust standard errors in parentheses. Constant, FE coefficients, and standard controls omitted from table.

Table 5: Controlling for Repression

	(1)	(2)
Budget balance t-1	0.537*** (0.049)	0.542*** (0.048)
Budget balance t-2	-0.074* (0.043)	-0.056 (0.041)
Budget balance t-3	0.118*** (0.040)	0.117*** (0.036)
Election	-0.009*** (0.003)	-0.008*** (0.003)
Election*Confidence	0.0002*** (0.0001)	0.0002** (0.0001)
Repression	-0.004** (0.002)	
Party bans		0.001 (0.002)
Observations	938	1,015

Note: Autoregressive OLS model with country and year fixed effects. Robust standard errors in parentheses. Constant, FE coefficients, and standard controls omitted from table.